THURSDAY, DECEMBER 27, 1883

VORTEX RINGS

The Motion of Vortex Rings. By J. J. Thomson. (London: Macmillan and Co., 1883.)

BOTH as regards the interest of the subject and the treatment it has received at the hands of the author we do not doubt that the essay before us is destined to take a foremost place amongst the essays which have been called forth, or at all events distinguished, by the Adams Prize.

The fact that these essays are upon set subjects precludes the possibility of the prize being awarded for a distinctly original conception. It is almost a necessity that the subjects chosen should involve the extension of some mathematical investigation which has already been carried a certain length.

The subject of the present essay is distinctly of this class; it involves an extension of the investigation of the theory of vortex motion in an ideal fluid, founded by Helmholtz and continued chiefly by Sir William Thomson.

At the time Helmholtz conceived the fundamental principle, ideal hydrodynamics had no other interest, besides its mathematical interest, than it derived from the somewhat casual explanations it affords of the phenomena met with in the motion of actual fluids. Helmholtz's investigation had some relation to the observed phenomena of actual vortices, particularly to the phenomena of smoke rings, of which it afforded a general explanation. But between the fundamental equations which Helmholtz gave and their application to an actual vortex ring certain integrations were necessary, and these integrations presented mathematical difficulties. If we consider the line of smoke which forms the ring as indicating the portion of air in which vortex motion exists, we may say that the difficulties of integration at which Helmholtz stopped arise from the thickness of this line of smoke, or, calling this the circular core of the ring, from the finite area of the section of this core. Helmholtz contented himself with applying his theory to an indefinitely thin core; and the fact that the results of a theory based on a frictionless fluid would only have an imperfect relation to the motions of viscous fluids, together with the fact that such rings, although they may be produced by artificial apparatus, are short-lived, and have no existence in the general motion of fluids, offered but little inducement for farther prosecution of the subject. The case however was altered when it was conceived by Sir William Thomson that the atoms of matter may be such rings moving in a perfect universal fluid. Smoke rings, although their behaviour seems to have suggested the idea, could not, owing to the viscosity of the air, by any means be made to afford an experimental verification of the capabilities of such an hypothesis. The only way was to integrate Helmholtz's equations, and thus arrive at the theoretical behaviour of such rings. Unfortunately the mathematical difficulties are such that there is little hope of obtaining a complete theory of vortex rings having cores of any finite area. Sir William Thomson, however, started an approximate theory as a step towards this; he succeeded in approximately integrating the equations for rings the cores of which had sections finite but small compared with the openings of the rings, and with such rings it appears that his theory can be tested as regards matter in the gaseous state.

To do this, however, it is necessary to do more than work out the theory of a single circular ring having a core of circular section. The phenomena of gases depend on the internal vibration of the atoms and on the influence which they exert on each other by collisions or otherwise. It was necessary therefore to obtain the theory of the vibrations of these rings, also of the effect of what may be called collisions.

Sir William Thomson took many steps towards the theory of vibrations. But the theory of collisions was left for Mr. J. J. Thomson.

Mr. Thomson has not, however, confined his attention to the point set for the prize, but, starting from the foundation laid by Helmholtz, has recast the theory to his own method.

Having deduced general expressions for the momentum, moment of momentum, and energy in a mass of fluid in which there is vortex motion, which expressions are better adapted for his purpose than any previously obtained, he proceeds to the theory of a solitary vortex ring subject to the same limitation as that treated by Sir William Thomson, *i.e.* the diameter of the core small compared with the opening of the ring, but of more general shape, in that it may have any small deviation from the circular form. He obtains results which, where they correspond, agree very approximately with those previously obtained by Sir William Thomson.

The author then proceeds to the immediate subject of the essay—the action upon each other of two rings.

In dealing with this subject he introduces another important limitation, *i.e.* that the rings shall not approach each other by a distance which is large compared with the openings of the rings.

With this limitation, by means of a very powerful piece of mathematical work, the theory of the mutual action of such rings is deduced, both as regards mean motion and vibration; and he has thus carried the theory of vortex atoms to such a stage that in certain general respects it can be applied to the theory of gases.

The essay, however, does not end here, for, although outside the set subject, the author proceeds to consider the theory of "linked rings." This term does not seem well chosen, for it conveys the idea of rings linked as in a chain, whereas what it is used to express is a ring of which the core is compounded of several separate cores wrapped in a spiral manner round each other like a ring composed of twisted wire.

In the treatment of this branch of his subject he has been no less successful than in the earlier parts.

From the general scheme of his essay it is clear that the author has had in his mind as a general object the verification of the vortex atom theory; and although he avowedly refrains from going at length into such a vortex atom theory of gases as might be built upon his work, he adds a chapter at the end in which he discusses certain results of his work, which may be applied without further calculation to the vortex atom theory of gases.

It is this chapter which will excite the most general interest, for although the fact of this still very incomplete theory being found consistent with observed gaseous phenomena would not afford a crucial test of its fitness to explain the phenomena of solids and liquids, still its failure to explain the phenomena of gases would appear to be crucial as regards its unfitness as an atomic theory.

The fair and cautious spirit in which Mr. Thomson discusses his results cannot be too much admired, although we may not be quite able to realise the truth of his reasoning.

The most general and important phenomenon of gases is that sometimes called Boyle's law—that the product of the volume and pressure of any fixed weight of gas varies directly as the amount of heat, *i.e.* kinetic energy, in a gas.

Accordingly Mr. Thomson calculates the product of the pressure and volume which would result in the case of a vortex atom gas. This he finds equal to two terms, one being the kinetic energy multiplied by a constant, the other a certain quantity which involves the squares of the velocity of the medium at the boundary surface. To fit Boyle's law this second term must vanish or nearly so. Mr. Thomson argues that it does so vanish, because the surface being at rest the velocity of the fluid at it must be small. This argument we entirely fail to follow, possibly owing to some misapprehension on our part; but it seems to us that a vortex being near a solid surface is no reason for supposing the tangential velocity of the fluid small, while if the gas consists of vortex atoms so must the solid surface, and there is nothing to show that the mean square of the velocity within the solid and at its surface will be less than in the gas.

Passing on from Boyle's law, with the explanation of which he is satisfied, the author next turns to the phenomena depending on the velocity of the gaseous molecules. As this seems to us the most interesting part of the discussion, we quote the passage in full:—

"According to the vortex atom theory, as the temperature rises and the energy increases the mean radius of the vortex rings will increase, but when the radius of a vortex ring is increased its velocity is diminished, and thus the mean velocity of the molecules decreases as the temperature increases; thus it differs from the ordinary kinetic theory, where the mean velocity and the temperature increase together. It ought to be remarked, however, that though in the vortex atom theory the mean velocity decreases as the temperature increases, yet the mean momentum increases with the temperature.

"The difference between the effects produced by a rise in temperature on the mean velocity of the molecules will probably furnish a crucial experiment between the vortex atom theory and the ordinary kinetic theory of gases, since all the laws connecting the phenomena of diffusion with the temperature can hardly be the same for the two theories. In fact, if we accept Maxwell's reasoning about the phenomenon called 'thermal effusion' we can see at once an experiment which would decide between the two theories.

"The phenomenon is this, if we have a porous diaphragm immersed in a gas, and the gas at the two sides of the diaphragm at different temperatures, then when things have got into a steady state the pressures on the two sides of the diaphragm will be different, and Maxwell, in his paper 'On Stresses in Rarefied Gases' (*Phil. Trans.* 1879, part i. p. 255), gives the following reasoning to prove that, according to the ordinary theory of gases, the pressures on the two sides are proportional to the square root of the absolute temperatures of the sides. He says:—

"'When the diameter of the hole and the thickness of the plate are both small compared with the length of the free path of the molecule, then, as Sir W. Thomson has shown, any molecule which comes up to the hole on either side will be in very little danger of encountering another molecule before it has got fairly through to the other side.

"'Hence the flow of gas in either direction through the hole will take place very nearly in the same manner as if there had been a vacuum on the other side of the hole, and this whether the gas on the other side of the hole is of the same or of a different kind.

"'If the gas on the two sides of the plate is of the same kind but at different temperatures, a phenomenon will take place which we may call *thermal effusion.* The velocity of the molecules is proportional to the square root of the absolute temperature, and the quantity which passes out through the hole is proportional to this velocity and to the density. Hence, on whichever side the product of the density into the square root of the temperature is greatest, more molecules will pass from that side than from the other through the hole, and this will go on till this product is equal on both sides of the hole. Hence the condition of equilibrium is that the density must be inversely as the square root of the temperature, and since the pressure is as the product of the density into the temperature, the pressure will be directly proportional to the square root of the absolute temperature.'

"If we were to apply the same reasoning to the vortex atom theory, we should no longer have the velocity proportional to the square root of the absolute temperature, but to some inverse power of it, and the above reasoning would show that if p and p' be the pressures, t and t' the temperatures on the two sides of the plate, $p/p' = (t/t')^m$, where m is a quantity greater than unity. Thus accurate investigations of the phenomenon of thermal effusion would enable us to decide between the vortex atom and the ordinary kinetic theory of gases. These experiments would, however, be difficult to make accurately, as we should have to work with such low pressures to get the mean path of the molecules long enough that the pressure of the mercury vapour in the air-pump used to rarefy the gas might be supposed sensibly to affect the results. In the theoretical investigation, too, the effects of the bounding surface in modifying the motion of the gas seem to have scarcely been taken sufficiently into account to make the experiment of the crucial test of a theory; and it is probable that the theory of the diffusion and viscosity of the gases worked out from the laws of action of two vortex rings on each other, given in Part II. of this essay, would lead to results which would decide more easily and more clearly between the two theories.

"The preceding reasoning holds only for a monatomic gas which can only increase its energy by increasing the mean radius of its vortex atoms; if, however, the gas be diatomic, the energy will be increased if the shortest distance between the central lines of the vortex cores of the two atoms be diminished, and if the radius of the vortex atom is unaltered the velocity of translation of the molecule will be increased as well as the energy; thus for a diatomic molecule we cannot say that an increase in the energy or a rise in the temperature of the gas would necessarily be accompanied by a diminution in the mean velocity of its molecules."

With the argument here used we have no fault to find, but it does seem to us that the author has fallen into some confusion between the experimental phenomenon of thermal transpiration through porous plugs and the theoretical idea of "thermal effusion." It has probably escaped Mr. Thomson, but the experiment he suggests

was included in the general investigation, made by the writer of the present review,[1] by which the phenomenon of thermal transpiration was discovered, and although it still appears that these are the only experiments on this subject, yet they conclusively prove that the difference of the pressure on the two sides of the plate is proportional to the square roots of the absolute temperatures. So far then it would seem that the crucial experiment has been made and that the verdict is against the vortex atom theory; but this is not so, for, although the experiment Mr. Thomson suggests has been made, it is definitely and experimentally shown in the same investigation that the action of the porous plug is entirely different from that which Maxwell calls thermal effusion, being due entirely to the tangential action of the walls of the passages, and further this tangential action is in strict accordance with the present dynamical theory of gases. This experiment with the porous plug, then, affords no test whatever in the way suggested by Mr. Thomson. Mr. Thomson has, we think, been unfortunate in his choice of tests; and we would suggest *the velocity of sound* as affording a crucial test for which the experimental work is already done. It appears to be an almost obvious deduction from the vortex atom theory that the velocity of sound must be limited by the mean velocity of the vortex atoms; and since Mr. Thomson has shown that this mean velocity diminishes with the temperature, while experimentally it is found that the velocity of sound increases as the square root of the temperature, it appears that the verdict must be against the vortex atom theory. However the vortex atoms are very slippery things, and we should like to hear Mr. Thomson's opinion before adopting one of our own.

Besides discussing the theory of gases, Mr. Thomson goes somewhat fully into a vortex atom theory of chemical combinations; in this he raises many points which will doubtless be of great interest should the hypothesis survive the crucial test by the theory of gases which this essay now for the first time renders possible.

Of the mathematical interest of the essay we can only say that to those who can appreciate it this will be found to be very great. OSBORNE REYNOLDS

OUR BOOK SHELF

Krystallographische Untersuchungen an homologen und isomeren Reihen. Von Dr. A. Brezina. I. Theil. Methoden. (Wien, 1884.)

THIS very useful volume forms an introduction to the author's crystallographic investigations which earned the prize of the Vienna Academy. It deals exclusively with the principles and the methods employed in those investigations, and constitutes a complete storehouse of the formulæ required in the study of crystals, and of the best means of applying those formulæ. The following subjects are successively treated: the optical principles involved in the goniometer; the practical use of the instrument, and the errors to which it is liable; the criticism of probable errors of observation; stereographic projection; all possible cases of trigonometrical calculation, including the method of least squares; and a slight sketch of the use of the polarising apparatus.

An important feature of the book is the illustration of methods by the actual measurement of seven crystals of a triclinic substance. The readings of the goniometer scale are first given, and from these the reader is led through the entire series of processes: stereographic projection, assignment of indices, calculation of elements, and recalculation of angles, each given in its place as an example of the principles and formulæ employed. This practical illustration is a far more effectual means of recommending the methods to the reader than mere verbal description.

It will probably be found that these methods of calculation are the most valuable part of the book; they are so systematically arranged and tabulated that the various steps may be distinguished at a glance, and any numerical error must be detected at once, while much labour is saved by the methodical order in which the operations are conducted.

It is to be presumed that the laborious process of calculating the angle between each pair of faces from the elements by means of the general formula is given as an exercise in the method of least squares rather than as an example of the course to be actually adopted in any but rare cases.

One subject, however, of some importance is barely touched upon; namely, the criticism of images obtained from crystal faces on the goniometer, and their interpretation. Both in the descriptive paragraphs and in the above-mentioned illustration, all measurements of the same angle upon different crystals are assumed to be equally good, so that their arithmetic mean is adopted as the observed value, whereas the difficulties presented by multiple images seem to deserve treatment in a book which deals so exhaustively with the practical side of the subject. It is to be regretted also that the discussion of optical properties and measurement has been almost crowded out of the work. H. A. M.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Remarkable Sunsets

SPEAKING of Virginia City, the great silver mining centre of Nevada State, I said, in "An Engineer's Holiday," that it "lies among the foothills of the Sierra, at an elevation of 6200 feet, on the eastern face of Mount Davidson . . . surrounded by innumerable interlocked mountains, conical in outline, red-brown in colour, and perfectly bare of all vegetation. These stretch, as far as the eye can reach, to where the snowy tops of the Humboldt peaks stand against the sky, and the terrible sterility of the scene is enhanced rather than relieved by the thin meanderings of the Carson River, whose course is marked by a narrow green line. This is the only sign of water visible in the arid panorama, whose bare, red cones are steeped all day in dust-haze, and lighted for a few minutes at sunset by an 'Alpenglow' which dyes the countless peaks in as countless gradations of rosy light."

It certainly did not occur to me, when I wrote the above three years ago, that the finer and higher particles of the dust-haze which obscures the dry air of the American desert may have been concerned in producing the splendid sunset effects which I witnessed at Virginia City; but this, after our recent experiences, seems very probable. D. PIDGEON

Holmwood, Putney Hill, December 22

I HAVE received a letter, dated December 5, from Mr. Joseph Moore, of New Garden, North Carolina, U.S.A., in which he informs me that "the phenomena at both sunset and sunrise have been unusual in more than a dozen instances here during the autumn. Only the night before last we had an extraordinary sunset. The sky bore all the tints of which you speak, but I do

[1] "Certain Dimensional Properties of Matter in the Gaseous State," *Phil. Trans.* 1879, Part II.

not remember to have noticed the cirrus cloud in more than one instance. The sunsets have been subject for remark in quite a number of the papers." I inclose also a newspaper, the *Olive Branch*, of Hancock, Minnesota, U.S.A., which has been forwarded to me by another correspondent, containing a notice of the sunset of November 10.

Richmond, Surrey, December 22 F. A. R. RUSSELL

IN a letter dated Tokio, October 3, describing a tour in the interior of Japan, Prof. James Main Dixon writes:—"During the two or three days at the end of August we enjoyed fine dry weather, but the sun was copper-coloured and had no brightness. It was capital weather for travelling, but rather inexplicable. When we got to Nikko, the people came to us to inquire if some catastrophe were impending, for the appearance of the sun foreboded evil. We laughed at their fears, and assured them all was right. However it seems that if the appearance of the sun foreboded no evil, it was a wonderful sign of the greatest earthquake and volcanic catastrophe on record. The fearful explosion of Krakatoa, in the Straits of Sunda, took place on August 26, and there seems little reason to doubt that the monsoon had carried the volcanic dust along with it, the dust obscuring the sun. The distance is nearly 3000 miles."

LEWIS CAMPBELL

St. Andrews, December 22

Peripatus

DR. VON KENNEL, in a note on the "Development of Peripatus," which appeared in a recent number of the *Zoologischer Anzeiger*, and has been translated and printed in your columns, has thrown some doubt on the accuracy of the observations recorded in the late Prof. Balfour's memoir on the "Anatomy and Development of *Peripatus capensis* (*Quart. Journ. Micro. Sci.*, April, 1883). We trust that you will give us, as the editors of that memoir, this opportunity of making a few brief statements in reply to the somewhat unusually outspoken criticisms contained in his preliminary note.

Dr. von Kennel entirely omits to mention in his paper that Prof. Balfour's researches refer to a Cape species of Peripatus (*P. capensis*), whilst the species which he has worked at are West Indian, and differ considerably from *Peripatus capensis*.

Considering the fact, well known to embryologists, that there are numerous instances of great discrepancies in the embryonic history of closely-allied forms, it seems to us strange that the only explanation, suggested by Dr. von Kennel, of the differences between his results and those recorded in Prof. Balfour's memoir should be that the latter are absurdly erroneous.

The remarkable attitude which Dr. von Kennel has assumed in this matter must have been obvious to all competent zoologists. We offer these remarks mainly because his statements have appeared in a journal which has a wide circulation amongst readers who are not so well able to judge of the merits of the case.

We are able to state in conclusion that the results enumerated on pp. 256, 257 of Prof. Balfour's memoir have been confirmed by Mr. Sedgwick on a large number of fresh and well-preserved embryos of Peripatus from the Cape, obtained since the publication of the memoir.

H. N. MOSELEY
A. SEDGWICK

[THE translator of Dr. von Kennel's "Note on the Development of Peripatus," to whom we submitted the above letter, writes to us that, "though with a large experience in such matters, he is quite unable to see anything 'unusually outspoken' in Dr. von Kennel's criticisms; had any such occurred, he would have passed them over; nor does he find any foundation for the statement that Dr. von Kennel explains the results of Prof. Balfour's memoir as 'absurdly erroneous.' Dr. von Kennel, at the beginning of his note, only asserts that his observations *cast some doubt* on those of Balfour, apologetically adding that his material was immensely richer than Balfour's, and at the conclusion of his Note he simply calls attention to the discrepancies between his observations and Balfour's illustrations." At the translator's request we quote the original of the two critical paragraphs with the translations, so that the many competent zoologists who are amongst our readers can judge whether the latter adds to or takes from the spirit of the former.—ED. NATURE.

"Ich thue dieses hauptsächlich deswegen, weil die durch Moseley und Sedgwick publicirte Abhandlung aus dem Nachlass Balfour's einige Abbildungen von Embryonen und Schnitten durch solche enthält, deren Genauigkeit ich nach meinem reichlichen und ausgezeichnet conservirten Material und nach den Beobachtungen am frischen Objecte etwas anzweifeln muss, deren Deutung vollends die Probe nicht hält."

"I do this chiefly because the treatise published by Moseley and Sedgwick from the posthumous notes of Balfour contains some representations of embryos and cross-sections of the same, upon whose accuracy in details I, with my rich and well-preserved collection of specimens, and observations on fresh objects, must cast some doubt, and the interpretation of which does not bear investigation."

"Ich enthalte mich hier, um nicht weitläufig zu werden, jeder Discussion, muss jedoch noch einmal darauf hinweisen, wie wenig Balfour's Abbildungen und die Schilderungen der Herausgeber mit den hier mitgetheilten Thatsachen stimmen."

"I here abstain for the sake of brevity from all discussion, but must, however, call attention to the fact how little Balfour's illustrations and the descriptions of the Editors agree with the facts as they are here given."]

A New Rock

DURING my visit last summer to Lake Sagvand, in the Balsfjord, near the city of Tromso, I discovered a new enstatite-bearing rock, which forms entire little hills. It is composed of light yellow-green enstatite, mixed with magnesite. The magnesite, which is entirely free from lime, is partly white, partly dirty grey in colour, in which latter state it contains a little oxidulated iron, and appears then distinctly crystalline, with rhomboidal planes of cleavage. The rock is greatly interspersed with little grains of chromite, which are found in the enstatite as well as the magnesite. Here and there small grains of pyrite also appear. The substance is perfectly free from olivine, at all events neither olivine nor serpentine has been discovered under microscopical analysis.

The rock must be considered a new petrographical species. I have named it "Sagvandite," from the place where it was first discovered. It appears with a strong reddish-brown colour on its uneven surface, where the magnesite is completely washed out, so that the enstatite alone remains. The rock is not slaty, and must so far be said to be of massive structure.

When I have had an opportunity of thoroughly analysing the new substance, I propose to give a complete description of it in NATURE. KARL PETTERSEN

Tromso Museum, Finmarken, Norway, December

Diffusion of Scientific Memoirs

IN his notice of the Reprint of Prof. Stokes' papers in NATURE for Dec. 13 (p. 145), Prof. Tait, with characteristic incisiveness, speaks of the "almost inaccessible" volumes of the *Cambridge Philosophical Transactions*, and proceeds to offer an "easy cure" for that simple though grave malady. I think if Prof. Tait had taken the trouble to make the inquiry he would have found that very few societies are so liberal in the free dissemination of their publications, and that the number of universities, prominent societies, or libraries which do not receive them gratis, or merely in exchange, is very small.

December 14 W. M. HICKS

THE question so pointedly at issue between Mr. Hicks and myself is one which can be settled by statistics only. NATURE would do a real service to science by collecting statistics as to the *numbers* of different centres (home, and foreign, separately) at which the *Transactions* of various scientific Societies were freely accessible in 1883 (say); and also the corresponding numbers in 1853. The Royal Society regularly publishes such information in its *Transactions*, so does the Royal Society of Edinburgh.

I have been a Fellow of the Cambridge Philosophical Society for about 30 years; and, during that time, I have received from the Society some fasciculi (of *Proceedings* only) certainly not amounting to a dozen in all:—and I am not aware that my case is an exceptional one.

Mr. Hicks writes as if he thought I was bringing an accusation. Surely the figure, of *malady*, which I was careful to employ, cannot be so construed. P. G. TAIT

THE "TALISMAN" EXPEDITION[1]

At the public meeting of the five Academies on October 29, 1882, I had the honour of reporting on the explorations of the *Travailleur*, and I announced that this year a new scientific campaign would be undertaken in the Atlantic. The Minister of Marine, responding to the desire expressed by his colleague, the Minister of Public Instruction, and by the Academy, had, in fact, issued the necessary orders to have the *Talisman* equipped for this purpose. The *Talisman* is an excellent screw steamer, provided with a good spread of canvas, sufficient to make good way without the aid of its engines. For several months it was placed in dock at the Rochefort Arsenal, where the naval engineers undertook to refit it for the service to which it had been appointed. The old hempen ropes intended for raising the dredges were replaced by a steel cable of great strength and flexibility, capable of a strain of about 4500 kilogrammes, and worked by two steam-engines. One of these set in motion the enormous bobbin on which the cable was wound. The other, a still more powerful engine, was intended for raising the dredges.

Large bag-nets, or trawls, with an opening of two or three yards, advantageously replaced the heavy drags we had formerly used. The soundings were executed by means of an apparatus perfected by M. Thibaudier, naval engineer, and so disposed as to prevent the motions of the vessel from in any way affecting the tension of the steel cable, which was arrested by an automatic break as soon as the sounder touched the bottom.

In order to gauge the temperature at great depths I had an apparatus constructed by which a mercurial thermometer (Negretti and Zambra) could be turned over at any moment. At the same time the capillary extremity of a glass tube, where a vacuum had been made, and into which the sea water then rushed, broke, supplying perfectly pure specimens, capable of being preserved for any length of time by soldering the tubes. Our friend, Colonel Perrier, had kindly lent me a Gramme machine, which generated the electricity for some Edison lamps, so disposed as to light up our apparatus, or, when needed, to penetrate to depths not exceeding 35 metres. At my request the command of the *Talisman* had been intrusted to M. Parfait, frigate captain, who had held the same position the year before on board the *Travailleur*.[2] I may here be permitted to express to the officers of the *Talisman* the feelings of gratitude inspired in us by their devotion. They cooperated with us with unflagging zeal, and for whatever success attended our mission we are indebted to them.

On May 30, the scientific mission[3] met at Rochefort, and on June 1 the *Talisman* set sail. The voyage of 1883 may be divided into several distinct stages. Our object was to study the coast of Africa as far as Senegal, then the waters of the Cape Verde, Canary, and Azores Archipelagos, volcanic lands which could not fail to supply us with interesting materials. Lastly, we hoped to be able to devote our attention to the Sargassum Sea, its fauna, and the nature of its bed.

The sea bed stretching westward of Morocco and the Sahara is extremely uniform, no longer presenting those rugged reliefs that had so impeded our operations on the coast of Spain. On the contrary, the slope is here so gentle that at greater or less distances from the land it was always possible almost infallibly to light upon the needed depths. In these waters we made about 120 dredgings, and in a few days we had determined the bathymetric distribution of the local fauna with sufficient accuracy to enable us to indicate the levels explored from the contents of our nets.

At 500 or 600 metres live numerous fishes, such as Macrurus, Malacocephalus, Hoplostethus, Pleuronectes, as well as prawns of the genus Pandalus, belonging to a new species with a rostrum pointed like a sword; some Peneæ, Pasiphaæ, a few small crabs (Oxyrhynchidæ, Portunidæ, Ebalidæ), pink Holothurians, some rare specimens of Calveria, that soft Echinoderm discovered in our waters by the naturalists of the *Porcupine*, and previously known in the fossil state; several very large sponges, such as Askonema and Farrea.

At greater depths, from 1000 to 1900 metres, fishes still abound,[1] and often formed the bulk of our captures. They were generally of a dull colour, with gelatinous flesh, and their skin covered with a thick mucous coating. Several had phosphorescent spots, serving to give them light in the dark regions they inhabit. Here Pandali give place to the new genus Heterocarpus, and to gigantic blood-red prawns with enormously long antennæ, which were new to science and may be placed in the genus Aristeus. The Nephropsis make their appearance at this level. They are blind, coral-tinted Crustaceæ, who seem to be distributed over a wide geographical range, for they have been found on the other side of the Atlantic in the Caribbean Sea, and a closely allied species has been fished up at a great depth near the Andaman Islands. The blind Polycheles, which in the present epoch represent the Jurassic Eryons, burrow in the mud, leaving nothing visible except their long hooked nippers, adapted for seizing the passing prey. Some crabs are still found, such as Maiadæ (Scyramathia, Lispognathus), a new species of Homolia, and Lithodes, hitherto supposed to be peculiar to Arctic and Antarctic seas. Lastly, numerous forms were also observed of the genus Galathea, several of which have their eyes transformed to spines. Sponges are extremely common, most of them with siliceous skeletons. We brought up great numbers of Rosella, Holtenia of several species, the rock crystal-like beards white as snow were buried in the mud, the sponge mass alone emerging; some Aphrocallistes, with solid skeletons of the most elegant form. Calveria became more numerous; Holothurians of the genus Loetmogone, and other species of the same family, crawled on the bottom in the midst of Asteria, Ophiuria, and Brisinga. The nets often returned filled with so much treasure that they could not all be classed within the day.

While rounding Cape Ghir and Cape Nun, some 120 miles from the coast, the *Talisman* spent several days in exploring a very regular bank at a depth of about 2000 to 2200 metres. It was on this ground that on Aug. 2, 1882, the *Travailleur* had captured the curious fish described by M. Vaillant under the name of *Eurypharynx pelecanoïdes*, and two specimens of which were taken this year.

Our prizes were again of great value. Magnificent sponges, allied to those that have been described under the name of *Euplectella suberea* were here found mingled with large violet Holothurians of the genus Benthodytes, and with other species of the same genus, remarkable for their dorsal appendices. A Calveria, distinct from those found at lesser depths, some Brisingæ, Polyps of rare beauty (Flabellum, Stephanotrochus), a Democrinus and a Bathycrinus, not yet described, very numerous Crustaceæ, nearly all new to us and belonging to the group of the Galatheæ (Galathodes, Galacantha, Elasmonotus), completed the list of invertebrates. The fishes were very varied, and their study will furnish new facts of the greatest interest to science. Amongst the most remarkable I may mention *Melanocetus johnsoni*, Bathy-

[1] Preliminary Report on the *Talisman* Expedition to the Atlantic Ocean. By M. Alphonse Milne-Edwards, President of the Submarine Dredging Commission. Communicated by the author.

[2] The staff consisted of M. Antoine and M. Jacquet, lieutenant, of MM. Gibory and Bourget, midshipmen, of M. Vincent, doctor of the first class, of M. Huas, assistant doctor, and of M. de Plas, chief mate.

[3] The mission consisted of M. A. Milne-Edwards, Member of the Institute, President, of M. de Folin, MM. Vaillant and Perrier, professors in the Museum, MM. Marion and Filhol, professors in the Faculty, M. Fischer, assistant naturalist in the Museum, MM. Ch. Brongniart and Poirault, added as assistants.

[1] To Macrurus are here added the following genera: Bathynectes, Coryphenoïdes, Malacocephalus, Bathygadus, Argyropelecus, Chauliodus, Bathypterois, Stomias, Malacosteus, Alepocephalus.

trochtes, a Stomias with phosphorescent spots, and several Malacostei.

Between Senegal and the Cape Verde Islands our trawls reached depths varying from 3200 to 3699 metres, and brought up most of the preceding species besides many others (Crustaceans, Mollusks, Zoophytes, Sponges) which had never elsewhere been met.

These last takes brought to a close the first part of our programme, and on July 20, after ninety-one days of navigation, we cast anchor in the Bay of La Praia, at Santiago in the Cape Verde Archipelago. This volcanic group detained us a few days, and while zoological, botanical, and geological excursions were being made ashore, the *Talisman* was searching the irregular beds on the coasts for marine animals, and especially for the red coral, which for some years back has formed the object of an active trade in these islands. I will not dwell on these in-shore explorations, nor on those of the islet Blanco, where we were able to study on the spot the large Saurians (*Macroscincus coctei*) which seem limited to this isolated rock.

All these details are recorded in the report which I have addressed to the Minister and which will soon be published.

In the deep waters of the Cape Verde Archipelago life displays a surprising energy. Our nets came up overflowing with specimens after a single plunge. We captured at one take more than 1000 fishes belonging mostly to the genus Melanocephalus, about 1000 Pandali, 500 prawns of a new species of the genus Nematocarcinus, with disproportionately long claws, as well as many other species.

On the evening of July 30 the *Talisman* took a northwesterly course in the direction of the Sargassum Sea. I need not enter into details on this part of our journey, and it will suffice to say that we nowhere met those dense floating masses of vegetation mentioned by the old navigators. The Gulf weed was seen in isolated patches drifting either with the marine or atmospheric currents, and harbouring a whole pelasgic population, whose colours harmonised admirably with those of the algæ that afforded them a refuge. Our naturalists made a careful study of these forms.

The soundings of the *Talisman* in this region show in a general way that, starting from the Cape Verde Islands, the marine bed falls regularly as far as about the 25th parallel, where it attains a depth of 6267 metres. Then it gradually rises towards the Azores and the 35th parallel, where it is about 3000 metres. These results are far from agreeing with the curves indicated on the most recent bathymetric charts. The bed of the Sargassum Sea seems formed of a thick layer of a very fine mud of a pumice nature, covering fragments of pumice and volcanic rocks. Here there would appear to stretch, at over three miles from the surface of the ocean, a vast volcanic chain parallel with the African seashore, and of which the Cape Verde Islands, the Canaries, Madeira, and the Azores are the only parts not submerged. The submarine fauna is poor, consisting of few fishes, some Crustaceans, such as Paguri, which lodge in colonies of Epizoanthus, prawns of the genus Nematocarcinus, Pasiphaæ, a few mollusks (Fusus, Pleurotoma, and Leda), which scarcely sufficed to repay the time required for such deep dredgings. Not that our captures did not again become abundant towards the northern limit of the Sargasse Sea, when the depths shrank to 3000, 2000, and 1500 metres. It was here that we took the giant of the family of the Schizopodes, a Gnathophausia of a blood-red colour measuring nearly 25 centimetres in length.[1]

A short delay at Fayal, and again at San Miguel in the Azores, enabled us to compare the volcanic phenomena still active at certain points with those we had studied on the summit of the Peak of Teneriffe. The analogy is very striking between the rocks, the gaseous products, and the sulphur deposits of the two islands. From what is now taking place on the surface of the ground, an idea may be formed of the submarine convulsions which have covered the bed of the Sargassum Sea with pumice and igneous rocks.

The return voyage from the Azores to France was effected under the most favourable conditions, and we were able to make daily dredgings in depths of from 4000 to 5000 metres. These difficult operations, very skilfully carried out by Captain Parfait, brought us an extremely valuable harvest. Under this tremendous pressure, in perfect darkness, and without a trace of vegetation, animal life is still vigorous. Large fishes of the genus Macrurus, as well as some Scopeli and Melanoceti seem to be here far from rare. Some Pagures and Galatheæ of new form, a gigantic Nymphon of the genus Colossendeis, some unknown Ethusæ, besides Amphipods and Cirrhipeds represent the Crustacean group. But this abyssal fauna owes its peculiar physiognomy to the number, variety, and size of the Holothurians.

The marine bed is carpeted throughout this region with a thick white mud, composed almost exclusively of Globigerini, and covering pumice deposits and fragments of various kinds of rocks. Some of these rocks brought up in our nets bore the impress of fossils, amongst others of Trilobites. But what still more surprised us was to find at a distance of over 700 miles from the European coast pebbles polished and striated by the action of ice. The sharpness of the striæ excludes the supposition of transport by the currents. The presence of these pebbles is probably due to the action of the icebergs, which in the Quaternary epoch advanced further southwards than at present, and which, by melting in the region of the Atlantic comprised between the Azores and France, deposited on the bottom of the sea the stones carried off from the glacier beds and conveyed to this distance from Europe.

On August 30 we dredged for the last time on the steep slope by which the oceanic depths are connected with the Bay of Biscay, and our captures added to the fauna of the French waters a large number of new or interesting species.

It was high time to return to Rochefort. Our casks and cases were full, our alcohol exhausted. This voyage has furnished us with unrivalled materials for study, materials which must now be put in order. The Minister of Public Instruction has recognised their importance, and has supplied me with the means of beginning the publication of the results. It is my intention to place before the public the collections that have been made during the explorations of the *Travailleur* and *Talisman*. These treasures will be exposed in a special exhibition, which will be held in one of the halls of the museum towards the beginning of January.

[1] *Gnathophausia goliath*, new species.

MUSIC AND SCIENCE[1]

It would seem that Science, like History, may at times repeat itself: for in this bright little pamphlet we have a revival of the Old World controversy, which dates from the days of Pythagoras, Plato, Aristotle, and Euclid. The author takes, however, for its text, a somewhat declamatory and *ad captandum* modern passage from the *Revue de Paris*, which declares, with an emotional warmth totally uncalled for under the circumstances, that harmony is not a science, and that music is an art, "but a divine art." To appreciate thoroughly the question in debate it is necessary to go back to the sense of the original Greek words—ἁρμονία and μουσίκη. The former means "mathematical agreement"; the second "artistic culture." It is with their "second intentions," or acquired and more limited meanings, that we now have

[1] "La Musica è una Scienza." Saggio Acustico fisiologico Del Dott. Primo Crotti. Pp. 55. Luigi Battei Editore. (Parma, 1883.)

to deal. Is music, in the English sense of the word, which no wise differs from the Italian, an art or a science? It is clearly both; but the art, μουσίκη, so far predominates in public acceptance and cultivation over the science, ἁρμονία, that the latter is, and has been for many centuries, in danger of succumbing altogether. Indeed, though excellently begun by Euclid in his "Sectio Canonis," it remained all but unadvanced until the recent researches of Helmholtz. It is to Aristotle that we owe the general test by which to distinguish an art from a science; a test so satisfactory and so neat, that it produces the effect on the mind of a mathematical demonstration; a form of proof which is too often only a roundabout way of re-stating a self-evident proposition. Aristotle said that art at its best only works by "rule of thumb"; and states that τέχνη is governed by rules. When these rules are found to rest on recognised laws, the art becomes an ἐπιστήμη, or science. This observation, made two thousand years ago by the shrewdest of all shrewd observers, remains as true and as fresh as on the day when it was promulgated. To no branch of human learning does it apply with such force and directness as to music. For perfection in this art has always been, is now, and must continue to be, confined to a few sensitive, delicate, finely-strung natures, which differ from those of their fellow-creatures in possessing a peculiar technical power and organisation such that they instinctively reproduce, and as it were consonate to the musical conceptions of other minds. In all other respects they may be self-indulgent, unbusinesslike, unpractical; even, as indeed not uncommonly they are, over-sensitive and disagreeable. Types of this class are Beethoven, Cherubini, Mozart, Weber, and Berlioz. In them, in fact, the full development of artistic perfection has eaten up all other good qualities, and left no time or inclination for what Plato calls "the practice of virtue." The world at large, secretly conscious of its special inferiority, and always willing to discharge itself of an unwelcome responsibility, too commonly looks upon these exceptional natures as representing the whole, and not only the artistic and executive side of music. But the other exists notwithstanding; and its fuller cultivation will tend much to restore the balance so disturbed. In this respect the little book of Dottore Crotti has special value. It deals with the foundation of rhythm and of music, and with the strange and hitherto unexplained emotional difference between the major and minor scales, which in the Italian are prettily and correctly named *Gaia* and *Triste* respectively. The ratios of musical intervals and their combination are fully treated, and with some features of novelty, especially as concerns their physiological effects on the ear. The great fact, so much forgotten in this century since the brilliant jigs of the Rossinian school have become popular, that it is the bass, and not the treble or melody, which is fixed and fundamental, is stated with abundant emphasis, and distinction is made between the characters of repose and of movement in different kinds of music. The assumption that the scale is founded principally on the fractions representing the major and minor tones with only a simple semitone of $\frac{16}{15}$ seems hardly sufficient to meet theoretical requirements; but otherwise there is much of interest comprised within the 55 pages of which the pamphlet consists. It has the merit, moreover, beyond the historical point already noted, of bearing out its title of "acoustico-physiological," and of adverting to the mental or receptive side of musical impressions more than occurs in some modern treatises.

W. H. STONE

THE REMARKABLE SUNSETS

THE following letter has been sent to Mr. Norman Lockyer:—

The remarkable sunsets which have been recently witnessed upon several occasions have brought to my recollection the still more remarkable effects which I witnessed in 1880 in South America, during an eruption of Cotopaxi; and a perusal of your highly-interesting letter in the *Times* of the 8th inst. has caused me to turn to my notes, with the result of finding that in several points they appear to have some bearing upon the matter which you have brought before the public.

On July 3, 1880, I was engaged in an ascent of Chimborazo, and was encamped on its western side, at 15,800 feet above the sea. The morning was fine, and all the surrounding country was free from mist. Before sunrise, we saw to our north the great peak of Illiniza, and twenty miles to its east the greater cone of Cotopaxi, both without a cloud around them, and the latter without any smoke issuing from its crater—a most unusual circumstance; indeed, this was the only occasion on which we noticed the crater free from smoke during the whole of our stay in Ecuador. Cotopaxi, it should be said, lies about forty-five miles south of the equator, and was distant from us sixty-five miles.

We had left our camp, and had proceeded several hundred feet upwards, being then more than 16,000 feet above the sea, when we observed the commencement of an eruption of Cotopaxi. At 5.45 a.m. a column of smoke of inky blackness began to rise from the crater. It went up straight in the air, rapidly curling, with prodigious velocity, and in less than a minute had risen 20,000 feet above the rim of the crater. I had ascended Cotopaxi some months earlier, and had found that its height was 19,600 feet. We knew that we saw from our station the upper 10,000 feet of the volcano, and I estimated the height of the column of smoke at double the height of the portion seen of the mountain. The top of the column was therefore nearly 40,000 feet above the sea. At that elevation it encountered a powerful wind blowing from the east, and was rapidly borne for twenty miles towards the Pacific, seeming to spread very slightly and remaining of inky blackness, presenting the appearance of a gigantic inverted ⌞, drawn upon an otherwise perfectly clear sky. It was then caught by a wind blowing from the north, and was borne towards us, and appeared to spread rapidly in all directions. As this cloud came nearer and nearer so of course it seemed to rise higher and higher in the sky, although it was actually descending. Several hours passed before the ash commenced to intervene between the sun and ourselves, and when it did so we witnessed effects which simply amazed us. We saw a green sun, and such a green as we have never, either before or since, seen in the heavens. We saw patches or smears of something like verdigris-green in the sky, and they changed to equally extreme blood-reds, or to coarse brick-dust reds, and they in an instant passed to the colour of tarnished copper or shining brass. Had we not known that these effects were due to the passage of the ash, we might well have been filled with dread instead of amazement; for no words can convey the faintest idea of the impressive appearance of these strange colours in the sky, seen one minute and gone the next, resembling nothing to which they can be properly compared, and surpassing in vivid intensity the wildest effects of the most gorgeous sunsets.

The ash commenced to pass overhead at about midday. It had travelled (including its detour to the west) eighty-five miles in a little more than six hours. At 1.30 it commenced to fall on the summit of Chimborazo, and before we began to descend it caused the snowy summit to look like a ploughed field. The ash was extraordinarily fine, as you will perceive by the sample I send you. It filled our eyes and nostrils; rendered eating and drinking impossible; and reduced us to breathing through handkerchiefs. It penetrated everywhere, got into the working parts of instruments, and into locked boxes. The barometer employed on the summit was coated with it, and so remains until this day.

That which passed beyond us must have been finer still. It travelled far to our south, and also fell heavily upon ships on the Pacific. I find that the finer particles do not weigh the 1/25,000 part of a grain, and the finest atoms are lighter still. By the time we returned to our encampment the grosser particles had fallen below our level, and were settling down into the valley of the Chimbo, the bottom of which was 7,000 feet beneath us, causing it to appear as if filled with thick smoke. The finer ones were still floating in the air, like a light fog, and so continued until night closed in.

In conclusion, I would say that the terms which I have employed to designate the colours which were seen are both inadequate and inexact. The most striking features of the colours which were displayed were their extraordinary strength, their extreme coarseness, and their dissimilarity from any tints or tones ever seen in the sky, even during sunrises and sunsets of exceptional brilliancy. They were unlike colours for which there are recognised terms. They commenced to be seen when the ash began to pass between the sun and ourselves, and were not seen previously. The changes from one hue to another, to which I have alluded, had obvious connection with the varying densities of the clouds of ash that passed; which, when they approached us, spread irregularly, and were sometimes thick and sometimes light. No colours were seen after the clouds of ash passed overhead and surrounded us on all sides.

I photographed my party on the summit of Chimborazo whilst the ash was commencing to fall, blackening the snow furrows; and, although the negative is as bad as might be expected, it forms an interesting souvenir of a remarkable occasion. EDWARD WHYMPER

December 21

NOTES

THE announcement that Prof. Flower has accepted the appointment of superintendent at the Natural History Museum, vacated by the resignation of Prof. Owen, is premature, though we believe that steps are being taken to secure Prof. Flower's services for that important appointment.

WE regret to have to record the death of M. Yvon Villarceau, one of the astronomers of the Paris Observatory and a member of the Academy of Sciences for more than twenty years. M. Yvon Villarceau had been a pupil of the École Central des Arts et Manufactures, and was regarded as one of the most eminent of French mathematicians.

IT has been arranged by H.M. Trawling Commissioners that Prof. McIntosh, of the University of St. Andrews, will proceed systematically at intervals (probably once a fortnight) to the trawling grounds on the east coast of Scotland for the next six months, and undertake certain investigations concerning the grounds and their inhabitants. Each trip will probably occupy about two days. The Granton General Steam-Fishing Company's steam-trawler *Wallace*, which is fitted with all the recent appliances for such work, and is a swift and powerful steamer, will be used for the investigations, which will be at once commenced. An experienced long-line fisherman and trawler from St. Andrews (Alex. W. Brown) will accompany the professor as assistant.

THE friends of the late Mr. W. A. Forbes, the Prosector of the Zoological Society of London, have decided to collect his most important papers in a memorial volume, and the following gentlemen have been appointed to act as a committee for this purpose:—Prof. Flower, Prof. Bell, Mr. H. H. Johnston, Mr. Mivart, and Mr. Sclater. The committee find that Mr. Forbes's papers can be most suitably republished in a form similar to that adopted in the memorial volume of the memoirs and papers of Mr. Forbes's predecessor in the Prosectorial office (the late Prof. Garrod). Following the precedent of the "Garrod Memorial Committee," they propose to ask for subscriptions of one or more guineas, and to give to subscribers a copy of the work for every guinea subscribed. Mr. Sclater will edit the Forbes Memorial Volume, Mr. Johnston will prepare a biographical notice and portrait, and Mr. F. Jeffrey Bell, 5, Radnor Place, Gloucester Square, W., will act as Secretary and Treasurer.

THE appointment of a Japanese student as assistant to the Professor of Anatomy at Berlin has been approved by the Minister of Public Worship.

MR. FRANCIS ELGAR, Consulting Naval Architect and Engineer in London, has been unanimously elected by the Glasgow University Court to the John Elder Chair of Naval Architecture.

AN expedition is at last being organised under the auspices of the British Association to proceed to Mount Kilimanjaro, the snow-clad peak of Eastern Equatorial Africa. The party will be under the charge of Mr. H. H. Johnston, who has recently returned from the Congo. The party will leave England at the beginning of March.

HEAVY indeed is the burden of educating laid upon the Southern States! With only one half at school of a population the illiterate proportion of which, among both whites and negroes, is increasing, and in some States this increase of illiteracy greater among the whites than among the negroes; with the negro, the non-taxpaying element, increasing fastest, notwithstanding white immigration; with trades destroyed, and property in consequence reduced in value 40 per cent., and in some States still falling in value; with the franchise, nevertheless, given to this increasing body of ignorance; evil indeed may be the result to a republic if the whole Union does not assist to correct it. Emancipation was a national act, and the nation ought to meet the inevitable consequence. So urges Dr. Haygood, in the United States educational circulars referred to last week, with the warning that no white men will agree for long to be voted down by a majority of illiterate blacks and whites.

ON Thursday, at 9.21 p.m., a shock of earthquake was felt in Fünfkirchen, a town in the south of Hungary, not far from the confluence of the Danube and Drave. The shock lasted two seconds, and was accompanied by a loud underground rolling noise. At the same time a similar earthquake and noise occurred at Barcs, a place to the south-east of the former, on the banks of the River Drave. Both shocks moved northwards. An earthquake shock was also felt at Lisbon at 1.30 a.m. on the 22nd inst., but did not excite much notice. A second shock, which lasted twelve seconds, occurred two hours later; being accompanied by subterranean rumblings, it awoke the entire population, and caused a panic among the inhabitants in the narrow streets. The seismic wave passed from north-west to south-east.

THE fourteenth Annual Report of the Botanic Garden Board of New Zealand (1883) contains valuable information as to the ravages of certain scale insects (*Coccidæ*) in the colony. They appear to be principally of two kinds: one is an *Icerya*, nearly related to the sugar-cane pest of Mauritius, &c., the other a *Mytilaspis* allied to the common "apple scale" (*M. pomorum*). The *Icerya* is called the "wattle blight," but appears by no means to confine its ravages to the wattle trees. According to Mr. Maskell, it is the *Mytilaspis* that is the more serious, for it overruns in countless millions all kinds of fruit and other trees (fortunately it appears to be enormously infested and destroyed by a parasite). With regard to remedies, there is a little vagueness in the Report, owing apparently to the confusion of the two insects. The first portion speaks only of the *Icerya*, and states that Mr. Engle of Nelson had completely destroyed it by the

application of kerosene and fish oil. Subsequently Mr. Maskell, dealing with the *two* species, says that a mixture of kerosene and linseed oil (one-third or one-fourth of the former) as recommended by Mr. Comstock in America, had been perfectly successful so far as regards the *Mytilaspis*, which he does not regard as serious in its probable effect upon wattles (*Acacia*), but very serious with respect to fruit and other trees. On the other hand, he considers all remedies useless against the *Icerya* of the wattle other than the radical one of cutting down and destroying the affected trees. No indication is given, however, of the use of a force-pump in distributing the kerosene; if this were used, the remedial agent might be distributed to a greater height than would be possible by mere hand application, and moreover it might be made to penetrate dense hedges, &c., the interior of which it would be impossible to drench by hand labour. The improved form of application, as a "kerosene emulsion," recommended by Prof. Riley and Mr. Hubbard, did not appear to be known in New Zealand at the time the Report was drawn up. Any way it is satisfactory to hear that the *judicious* application of kerosene will certainly destroy scale insects without necessarily damaging the plants.

THE same Report speaks very hopefully of the ultimate success of attempts to cultivate hops in the province of Wellington; in Nelson success has been already secured. The great drawback is the expense of providing the necessary *poles*, and much stress is laid upon the necessity for cultivating oak, ash, birch, and species of *Eucalyptus* for that purpose. Of the indigenous poles, those of *Myrsine urvillei* are said to be the most durable.

THE additions to the Zoological Society's Gardens during the past week include a Toque Monkey (*Macacus pileatus* ♀) from Ceylon, presented by Mr. J. H. Barker; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. Douglas; a Common Marmoset (*Hapale jacchus*) from Brazil, presented by Mrs. Archer; a Moorhen (*Gallinula chloropus*), British, presented by Mr. T. E. Gunn; a Gannet (*Sula bassana*), British, presented by Mr. J. C. Baxter; two St. Thomas's Conures (*Conurus xantholæmas*) from St. Thomas, West Indies, presented by Mr. C. Wallis Enslie; two Fringed-lipped Lampreys (*Petromyzon branchialis*), British, presented by the Rev. F. T. Wethered; a Pied Wagtail (*Motacilla lugubris*), British; a Slaty Egret (*Ardea gularis*), European, purchased.

INTERNATIONAL POLAR OBSERVATORIES

I BEG to inclose you an extract from a letter just received from Prof. Wild, President of the International Polar Committee, and which gives information as to the several expeditions which conducted observations in the circumpolar regions during the twelve months ending August 31, 1883.

ROBERT H. SCOTT

"I take this opportunity of stating concisely what I have hitherto learnt as to the present condition or the return of the various expeditions.

"1. The United States—Point Barrow.—The Expedition was to have returned in the summer of 1883. Definite information as to its return has not yet been received.[1]

"2. England and Canada—Fort Rae, on the Great Slave Lake.—According to a communication received from Mr. Scott, dated November 21 last, the Expedition has safely returned to England.

"3. United States—Lady Franklin Bay.—The attempts to relieve the Expedition this summer by ship have, like those of last year, failed owing to the unfavourable condition of the ice. (Extract from newspapers.)

"4. Denmark—Godhavn, in Greenland.—According to a communication from Captain Hoffmeyer, dated December 8, the Expedition has safely returned to Copenhagen with a rich store of observations.

"5. Germany—Cumberland Sound (Davis Strait).—According to a communication received from Dr. Neumayer, dated November 1, the Expedition has safely returned to Hamburg, having completed its task in a satisfactory manner.

"6. Count Wilczek's Station (Austria)—Jan Mayen, in Marymuss Bay.—The Expedition has safely returned to Vienna, having completely carried out its programme. A short report of its operations has been published by M. von Wohlgemuth, the Chief of the Expedition.

"7. Sweden—Spitzbergen (Cape Thordsen, in the Ice Fjord).—Dr. Rubenson states that the Expedition has safely returned to Stockholm.

"8. Norway—Bossekop, near Alten.—From a letter from Prof. Mohn, dated September 7, the Expedition stopped work on August 31, having completely carried out its programme, and on September 17, according to a report in *Naturen* (October, 1883) it safely returned to Christiania.

"9. Finland—Sodankyla.—The Expedition completed its task for the first year, but, according to a communication from Prof. Lemström, dated August 5, the observations will be continued another year, as the Government of Finland has provided the funds for the purpose.

"10. Russia—Nova Zembla (Möller Bay).—The Expedition returned to St. Petersburg in October with a rich store of observations.

"11. Holland—The Kara Sea.—The Expedition could not reach its original place of destination, Port Dickson, but was surrounded by ice in the Kara Sea, and has, according to a letter from Prof. Buys Ballot, dated October 1, safely returned to Utrecht, having under the circumstances only imperfectly carried out its programme.

"12. Russia—Mouth of the Lena (Sagastyr).—The Expedition, which suffered from storms during the passage down the Lena, was not properly established until October 20, 1882; from that date it has been able to carry out all the work laid down in the programme. It will continue its observations for another winter.

"13. France—Cape Horn (Orange Bay, Terra del Fuego).—According to a report from Prof. Mascart, dated November 17, the Expedition has returned safely to Paris, with a rich store of materials.

"14. Germany—The Island of South Georgia (Moltke Harbour).—This Expedition has also safely returned, according to a communication from Dr. Neumayer.

"Of the fourteen Expeditions, therefore, three will continue their observations for about another year (Lady Franklin Bay, Sodankyla, and Lena delta); the continuance of a fourth (Point Barrow) is at present unknown, the other ten have safely returned."

[1] We believe this party arrived at San Francisco some weeks ago.—ED.

MOVEMENTS OF THE EARTH[1]

III.—*Rotation of the Earth*

THE several ideas concerning the movements of the earth which were introduced in the last lecture will in the present one have to be dealt with in greater detail.

It was then agreed that if the whole expanse of the heavens were to travel with a perfectly equable motion in one direction, such a motion for instance as would result from all the stars being fixed to a solid transparent substance like those crystal spheres that the ancients really believed to exist; or if, on the other hand, the earth herself, instead of being free to turn as she listed with varying velocity in any direction, really went with perfect constancy in the direction opposite to the apparent motion of the stars, the visible effects would be the same in both cases, so that an appeal to our eyes would not suffice to enable us to say whether the earth moved or whether she remained at rest while the celestial sphere revolved around her.

Under these circumstances what is to be done? It has been seen how, both with regard to the measurement of space and the measurement of time for astronomical purposes, those interested in the physics and beauties of the various classes of celestial bodies outside our own earth have picked and chosen now one bit of physical science and now another to help them in their inquiries; and with regard to this very important question, "Does the earth move or is she at rest?" we shall see how very beautifully and perfectly the question has been answered by the application of certain mechanical principles.

The majority of people, I suppose, have some acquaintance, however slight, with machinery—with steam engines for in-

[1] Continued from p. 69.

stance; and it is a familiar fact how very important a part is played in the steam-engine by the flywheel. Why should that be? Why should this flywheel be so important that it is only quite recently that mechanicians have learned to do without it? For this reason: if a mass of matter such as a flywheel is once made to revolve, it will retain that motion for a long time, resisting any tendency to an increase or decrease of its velocity. It is in consequence of this property which the revolving flywheel possesses that an engineer is able to get over the dead points in his engine, whilst

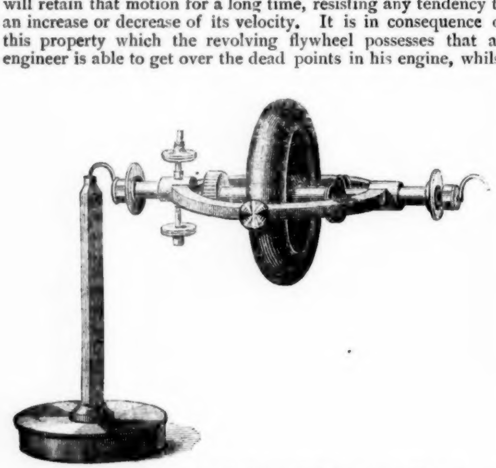

FIG. 27.—Rapidly rotating wheel supported at one end of its axis.

it also acts in preventing the engine making too sudden a start. In addition to this, when we have a mass of matter in the condition of the revolving flywheel it has some very peculiar qualities, only observed when such a mass of matter is in motion. If, then, we have a wheel so arranged that a very rapid rotation is being imparted to it, it does not behave as it would when at rest. These properties possessed by a rotating body can be well shown by an instrument known as the gyroscope, of which we shall speak more fully later on. It consists essentially of a

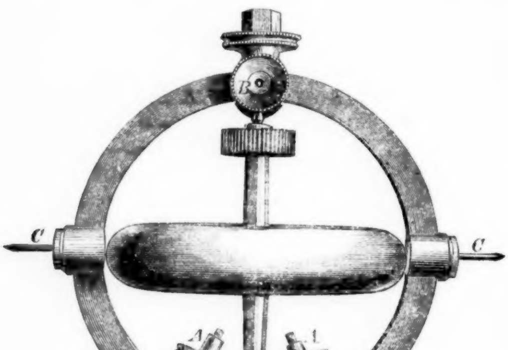

FIG. 28.—Rotating disk of gyroscope. C C, knife edges; A A, B B, adjusting weights.

disk to which a very rapid rotation can be imparted by a train of wheels or by other means. If the disk be set rotating, it is found to possess those curious qualities of which I have spoken. If whilst rotating at a high velocity it be placed in the position shown in Fig. 27, it will not fall, but will take on a movement of revolution round the stand.

From considerations suggested by this and other similar experiments, Foucault pointed out that it might be demonstrated whether the earth moved or whether she remained at rest. It struck him that the problem should be attacked somewhat in this manner:—

Suppose the earth to be at rest, and that either at the north or south pole a pendulum, suspended so that its point of support had as little connection with the earth as possible—so that it should, in fact, like the rotating flywheel, be independent of external influences, were set vibrating. Then an observer at the north or south pole would note that the swinging pendulum (the earth being considered as at rest) always had the same relation to the objects on his horizon. But, said Foucault, suppose that the earth does move. Then the swing of such a pendulum would not always be the same with regard to the places on the observer's horizon. Let the earth be represented by a globe. Suppose it to rotate from west to east. Place it with the north pole uppermost, and set the pendulum, whose point of support is disconnected from the rotating earth, vibrating. Then the pendulum will appear to travel from left to right as the earth rotates from right to left beneath it. Now suppose the pendulum to be suspended in the same way at the south pole, right and left now being changed. The earth of course rotates in the same direction as before, but the pendulum now appears to change the

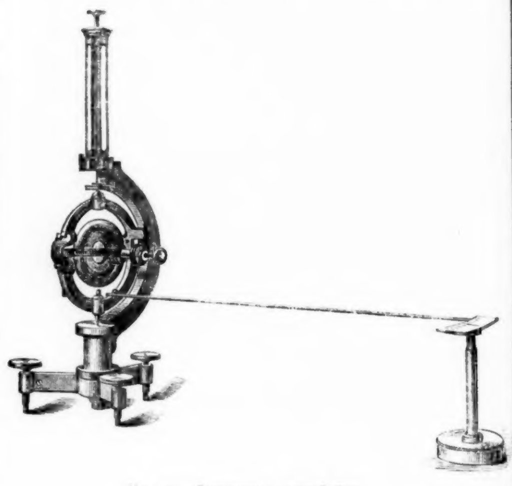

FIG. 29.—Gyroscope; general view.

plane of its swing from right to left. At the equator the earth simply rotates straight up and straight down beneath the swinging pendulum.

From these considerations it became evident to Foucault that, if there were any possibility of demonstrating the movement of the earth by means of the pendulum, the demonstration would take this form. Provided it were possible to swing a pendulum so that it should be as free as possible from any influence due to the rotation of the earth, and take that pendulum to the north pole, it would appear to make a complete swing round the earth in exactly the same time that it really takes the earth to make a complete rotation beneath it. At the south pole exactly the same thing would happen except that the surface of the earth would appear to move in the opposite direction to what it did at the north pole. Now it will be perfectly clear that if we thus get a pendulum appearing to swing one way on account of the true motion of the earth at the north pole and in the opposite direction on account of the true motion of the earth at the south pole; at the equator, as we found in dealing with our model earth and model pendulum, it will not change the plane of swing either way, that is to say, the time taken by a pendulum to make a complete swing will be the smallest possible at the poles, whilst at the equator it will be infinite.

At all places, therefore, between either pole and the equator

the period of swing will be different, and the time taken to make a complete swing will increase or decrease as the equator is approached or receded from. So much for theoretical considerations. Can they be put to the test of experiment, and an answer obtained from nature herself? The fact is that this idea of Foucault's is so beautifully simple that anybody can make the experiment providing he has the means of using a very long pendulum. This pendulum must be rigidly, but at the same time very independently, supported.

Beneath the pendulum, in contact with the earth, and therefore showing any movement of rotation which the latter may possess, is a board, on the centre of which the pendulum nearly rests. From the central point of this board lines are described showing so many degrees from the central line over which the pendulum bob swings. These preliminaries being arranged, let the pendulum be started. This is done by drawing it out of the vertical and tying it by a thread which is burnt when it is desired to start the experiment.

Then, in consequence of that quality the existence of which was revealed to us by the rotating disk and which is possessed by this vibrating pendulum, and in consequence of the precautions which have been taken to prevent its swing being interfered with by the motion of the earth or other perturbing influences, it should be found, if Foucault's assumption be correct, that the earth is moving beneath the pendulum. And if all the conditions of the experiment have been complied with it is found that the pendulum moves over the scale as the earth rotates beneath it. That then is one demonstration of the existence of the earth's rotation.

The question now arises whether there be any other method of determining the same thing. There is, but in answering the question in the affirmative it must be said that this second method is neither so simple nor so satisfactory as the first.

We owe it also to the genius of this same man, Foucault. It depends upon the same principles and is connected with the same series of facts as the other. But before proceeding to

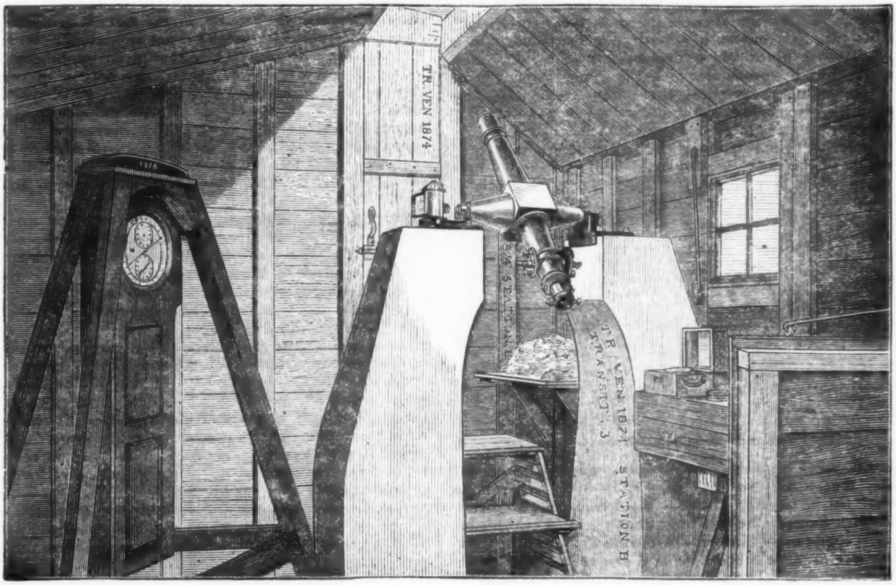

FIG. 30.—Transit instrument and clock.

discuss this second experiment it will be well to consider these two tables, which have been taken from Galbraith and Haughton's "Astronomy," because they show not only what the swinging pendulum should do if it behaves properly, but also what the gyroscope, the instrument used in the second experiment, should do if it behaves properly.

The first table is called

Hourly Motion of Pendulum Plane.

Place	North Lat.	Observed motion per hour	Calculated motion per hour	Observer
	° ′	°	°	
Ceylon	6 56	1·870	1·815	Schaw and Lamprey.
New York	40 44	9·733	9·814	Loomis.
Providence, R.I.	40 49½	9·955	9·833	Carswell and Norton.
New Haven, Ct.	41 18½	9·970	9·929	
Geneva	46 12	10·522	10·856	Dufour and Wartman.
Paris	48 50	11·500	11·323	Foucault.
Bristol	51 27	11·788	11·763	Bunt.
Dublin	53 20	11·915	12·065	Galbraith and Houghton.
Aberdeen	57 9	12·700	12·636	Gerard.

The second is

Rotation of Earth deduced from Pendulum.

Place	Time of Rotation		
	h.	m.	s.
Colombo, Ceylon	23	14	20
New York	24	8	9
Providence, R.I.	23	38	29
New Haven, Ct.	23	50	7
Geneva	24	41	39
Paris	23	33	57
Bristol	23	53	2
Dublin	24	14	7
Aberdeen	23	48	49
Mean value	23	53	0

The pendulum plane is of course the plane in which the pendulum swings. The first column in Table I gives the place where the pendulum was set swinging, the second the latitude,

the third the observed motion per hour, and the fourth the calculated motion. The table has been so drawn up that it begins with places nearest the earth's equator and passes gradually to others further away, going from Ceylon at 6° N. lat. to New York at 40° N. lat., New Haven at 41°, and ending with Aberdeen at 57°. At the first-named place it will be seen that the pendulum swings through less than 2° per hour, whilst at Aberdeen it swings through nearly 13°, which is an approximation, at least, to the statement I have made, that, since the rotation of the pendulum plane will be most rapid at either pole, the further from the equator we swing it the greater will be the number of degrees passed over per hour.

To turn now to the gyroscope. We shall expect, if we succeed in imparting to it a rotation which is independent of and unaffected by the earth's rotation, that the angular change shown by it will be the same as that indicated by the pendulum, or, in other words, that the number of degrees passed over will be the same in both cases.

In the gyroscope, that portion which corresponds to the swinging part of the pendulum is the heavy disk seen in Fig. 28, to which a very rapid rotation can be imparted. This disk is mounted upon the horizontal circle shown in the figure, which circle in its turn is mounted in a vertical one suspended by a bundle of raw silk fibres which depend from the little screw shown at the top, by means of which the whole system can be raised, so preventing the vertical circle from resting its whole weight upon the pivot below, the use of which is not so much to support the apparatus as to guide it in its movements.

Now in order that the rotation of the disk shall be uninfluenced by the motion of the earth a great number of precautions have to be taken. The first of these is to insure that the whole of the apparatus shall be perfectly free to rotate, and that, however

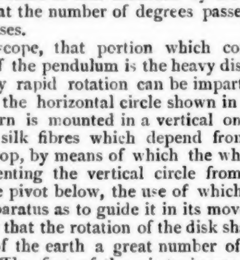

FIG. 31.—Wires in transit eyepiece.

much the silk fibres supporting the vertical circle may be screwed up in order that it may not rest its weight upon the pivot, its motion shall not be interfered with—that there shall be no twist in the thread. This is the first precaution; and, when this has been done, a condition of things is obtained in which the apparatus is perfectly free to move round a vertical axis represented by the silk fibres prolonged. Then, having fulfilled this condition, the next matter of importance is to see that the disk is perfectly free to move on the horizontal axis. For this purpose the wheel which holds the two extremities of the axis of the rotating disk is armed with counterpoise weights (see Fig. 28), two in a horizontal plane, A A, and two in a vertical plane, of which one is seen at B.

Then the knife edges, C C, which are exactly in the plane of the centre of motion of the whole system, are made to rest on two steel plates mounted on a separate stand, in order to ascertain if the moving parts are perfectly balanced, the perfection of balance being determined by the slowness with which it oscillates up and down. But this is not all; it must not only be so adjusted by these weights, A A, that the ring shall remain horizontal, but it must be so perfectly balanced by the two weights, one of which is seen at B in Fig. 28, that if a considerable inclination be made from the horizontal it will be taken up equally on both sides. Finally, the instrument must be so adjusted that when the two delicate knife edges are placed on the two steel plates in the outer ring (see Fig. 28) the ring carrying the disk shall be perfectly free to move and have its centre of motion exactly identical with the centre of motion of the outer ring and of the disk itself. Then, when all these precautions have been taken, and the disk is set rotating with considerable velocity by means of a multiplying wheelwork train, we have, as far as the mechanics of the thing are concerned, an experiment just like the other, with this important difference, however, that, whereas the pendulum experiment always succeeds, much trouble is often experienced in experimenting with the gyroscope. But, when the multiplicity of the conditions necessary to the success of the experiment is considered, this is not surprising. If, however, all the conditions have been adhered to, the pointer with which the instrument is fitted (see Fig. 29) ought to move over the scale at exactly the same rate that the pendulum moves over the scale beneath it. But even supposing that the pointer of the gyroscope does move over the paper and in the right direction when the apparatus rotates one way, this is not enough. The demonstration of the validity of the result given by it is that an equivalent deviation is obtained when the apparatus is turned about in every possible direction. The first test of course is to rotate in the opposite way, then, if all the adjustments have been properly made, the deviation obtained will be the same in amount and direction as before, and it may be taken that the result obtained is then really due to the earth's rotation.

With this reference to the most important points connected with the gyroscope, we may bring our inquiries under this head to a close. So many men have worked with the instrument in so many lands, and under such rigid conditions, that there can be no doubt that the rotation of the earth is demonstrable by it, although certainly its verdict is not anything like so sharp, or so clear, or so easily obtained, as that given by the pendulum.

Our appeal to physics has at once put out of court the old view of the arrangement of the universe, which placed an immovable earth at its centre. How Copernicus was the first to point out that this old view was incorrect, and that it was the earth which moved, and how Galileo was persecuted because he, in times much less fortunate than our own, had the courage to say so,—these are familiar points in the history of the discovery of the earth's rotation.

Having then demonstrated the existence of this particular movement of the earth, we must now proceed to a consideration of the rate, direction, and results of the movement,—connect in fact the pendulum of Foucault with that of Huyghens, and regard the physical pendulum as giving an important use to the experiments of Galileo and of Huyghens in which they caused it to act as a controller of time.

Turn back to our two tables. They are not without interest at the present moment. In the first table, "Hourly Motion of Pendulum Plane," the observed motion of the pendulum plane per hour is connected with the latitude of the place at which it swings, varying as that varies; and therefore the observed motion in any latitude ought to give the same value for the earth's rotation, the closeness of which to the real value will at the same time be a measure of the accuracy of our pendulum observations.

Let us endeavour then to find out in what time the earth must go round in order that the pendulum plane may vary (say) $1\frac{8}{10}$° per hour in Ceylon, $11\frac{0}{10}$° in Dublin, and so forth.

Taking our clock as being divided into twelve hours, each hour into sixty minutes, and each of these again into sixty seconds, it is found (see Table 2) that the value for Ceylon is 23h. 14m. 20s., and for Dublin 24h. 14m. 7s., the mean value of the observations at the various places mentioned in the table being 23h. 53m., so that according to that table the earth rotates on its axis in a few minutes less than twenty-four hours.

Now although such an approximation to the real value may suffice for the great mass of mankind, it is not an astronomical way of dealing with the question. We have seen the circumference of a circle divided first into degrees, then into $\frac{1}{3}$ degrees, next into seconds, and finally into tenths of seconds; by the application of electrical principles, time has been even more finely divided, and the question naturally arises, Are there any means of determining the exact period of the earth's rotation?

There are means of doing this. In the last lecture occasion was taken to point out that the stars are infinitely removed from the earth; the stars being so infinitely distant, a slight change in their position will not be perceptible to an observer on the earth, and the place of a star to-day and its place to-morrow are the same so far as relates to any parallactic change of position.

This being premised, it will be clear that, in order to get out the exact period of the earth's rotation, one only has to make an observation of any star on one particular day (such observation being of course made with a clock), and repeat the observation when the star is in the same position on the succeeding day. The time which elapses between the observations must be the time taken by the earth to make a complete rotation. But it

will be asked, How are these observations made, and how is it known when the star *is* in the same position when the second observation is made?

For this purpose a transit instrument is used (see Fig. 30). This differs from an ordinary telescope, being so mounted as to move only up and down, and is armed not with simple cross wires, but with an odd number of parallel and equidistant vertical wires crossed by a single horizontal wire. It is also usually provided with a circle to give declination. If from any part of the earth an observation be made on any particular star on one day, and then another observation made on the same star when it is in the same position the next day, as has been said, the interval between the two observations must be the time taken by the earth to move round once.

By having such an arrangement as exists in the transit instrument, by which it can swing in the plane which coincides with the axis on which the earth turns, any star may be chosen for the observation. Suppose, for instance, the instrument be pointed to the north pole star, then, in consequence of the tremendous distance of the stars, the axis of the telescope is practically coincident with the axis of the earth. But suppose another star to be observed, it will be quite clear that we may make the observation on it, or any other star we choose. When the instrument is upright it points to the zenith. A star in the zenith may therefore be selected for the observation.

FIG. 32.—Showing that the true horizon of a pole is the equator.

It is observed when crossing the central wire of the instrument one day, and noted again when it crosses that wire on the succeeding day. But the observer does not limit his observation to the one central wire, in order to ascertain when the star is in the centre of the field. If he did so, he might miss his observation. That is why the simple cross wires have been replaced by a system of wires (see Fig. 31). As the star crosses the field of view, the observer, listening to the beats of the clock alongside, notes the time when it crosses each of the wires, and takes the mean of these observations, thus attaining to a much greater accuracy than if he had merely observed the transit over the central wire. With an ordinary clock it is found that a period, less by a few moments than twenty-four hours, elapses between two successive transits.

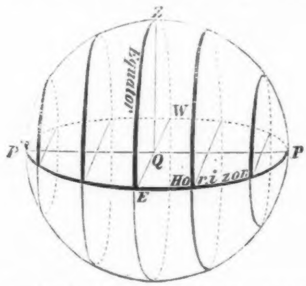

FIG. 33.—Showing that the poles lie in the horizon at the equator.

In order to get an absolutely perfect measure of time, the clock may be so rated that it should not be any indeterminate number of hours, minutes, and seconds, but twenty-four hours exactly between the two transits of that star. With a clock thus arranged, the time at which a star crossed the central wire of the transit instrument would really give a most perfect method of determining that star's place in the heavens, because, if the earth's rotation is an equable one and takes place in a period which we choose to call twenty-four hours, then two stars 180° apart will be observed twelve hours after one another, four stars 90° apart will be observed six hours apart, and so on; and clocks like this, regulated to this star time, exist in our observatories, being called sidereal clocks, because the time they give, which is not quite familiar to everybody, is called sidereal time.

Now let us consider our position on the earth with regard to the stars. This is a very interesting part of our subject, not only in its scientific aspect, but from the point of view of its usefulness, whether we wish to study the stars or define places on the earth's surface, the latter matter, however, being so intimately connected with astronomy proper that it is impossible to talk about the one without talking about the other.

Since we divide all circles into 360°, the circumference of the earth may be so divided, and the method in use of defining positions on the earth is to say of a place that its latitude is so much and its longitude is so much. Latitude begins at the equator with 0°, and terminates at the poles with 90°, being north latitude in the one case, and south latitude in the other. In the case of longitude, there is no such simple starting point, for whilst latitude is counted from the equator by everybody all over the world, longitude may commence at any point. In England we count longitude from the meridian of Greenwich. When the transit instrument at Greenwich is swept from the north point through the zenith to the south point it describes a half circle, which is called the meridian of Greenwich.

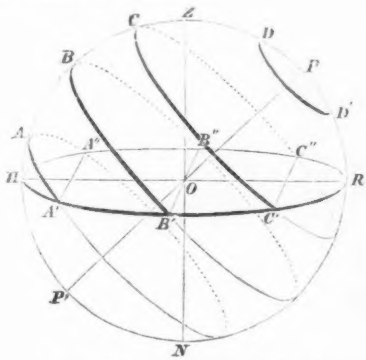

FIG. 34.—Horizon of a place in mid-latitude.

That is one point. Another point is this. Suppose the instrument to be set up not at Greenwich but at the north pole. Then the true horizon of the observer will be along the equator. Remove the instrument to the equator, and the true horizon will cut the poles. At a place in mid-latitude the true horizon would cut neither the pole nor the equator, but would be inclined to both (see Figs. 32, 33, and 34).

Then comes the important relationship between the latitude of the place and the altitude of the pole star above its horizon; that the number of degrees this star—be it north or south—is above the horizon of the observer will be the number of degrees of north or south latitude of the place where the observation is made. A place therefore in 10° N. lat. will (roughly) have the north pole star at a height of 10° above its horizon.

So much for this part of our subject. Let us now leave it, because, interesting as it is, it refers to a branch of astronomy with which at present we have less to do than with the more physical one; but it was well that we should pause for a few moments to note the tremendous importance to mankind of that particular movement of the earth which we have been considering.

J. NORMAN LOCKYER

(To be continued.)

PROBABLE NATURE OF THE INTERNAL SYMMETRY OF CRYSTALS[1]

THE theory of the modification of crystal angles, just offered in dealing with quartz, is manifestly applicable to all crystals not of the cubic system, and it is submitted that for every such

[1] Continued from p. 188.

crystal there is an *ideal* or *root form* proper to one or other of the five kinds of internal symmetry which have been presented, from which root form the actual form can be derived by a proper proportionate increase of dimension in one or more directions.

It is evident that, while our path must become more and more intricate as we endeavour to establish in the cases of more complex compounds relations similar to those above traced, the reference of whole classes of analogous forms, differing only in their angles, to one root form, removes a very important difficulty, and the wide applicability which it confers on the five kinds of internal symmetry with which we started appears in the fact that there is no crystal form which cannot be thus referred to an appropriate root form in harmony with one or other of these five kinds of internal symmetry.[1]

One more case may be mentioned in which a probable internal symmetry can be assigned to a compound in harmony with its actual crystal form; it is a more difficult one.

The molecule of *Iceland* or *calc-spar* is usually believed to consist of one atom of calcium, one atom of carbon, and three of oxygen. We shall, however, take a liberty, and suppose that the atoms of calcium or the atoms of carbon have but half the mass attributed to them; that in the formula of this compound we should write either two atoms of calcium or two atoms of carbon in place of one.[2]

Making this supposition, we observe that if the calcium and carbon atoms were alike we should have six atoms, three of one kind, three of another; in other words, we should have equal proportions of two kinds of atoms, from which, since the form of Iceland spar is but little removed from a cube, we naturally argue that just before crystallisation its atoms were arranged according to the first or second kind of internal symmetry; these two kinds being, it will be remembered, those in harmony with the cubic form which admit of very symmetrical arrangement of particles of two kinds present in equal numbers.

Since Iceland spar is a uniaxal crystal, the arrangement of the three kinds of atoms, whatever it is, must be symmetrical about one axis only; and we shall now endeavour to show that the atoms can be thus arranged in either the first or second kind of symmetry.

We will show first that they can be thus arranged in the second kind.

Where there are but two kinds of particles present in equal numbers, symmetry requires that the alternate layers of this kind of symmetry (see Fig. 3) shall consist entirely of similar kinds, and therefore in the case before us, one set of alternate layers will represent oxygen atoms; the other, atoms of calcium and carbon. Now particles present, as we suppose the calcium and carbon atoms to be, in the proportion 1 : 2 can be quite symmetrically arranged in these layers (plan *f*), as the sphere centres were in the layers depicting the fourth kind of symmetry (plan *e*), and therefore the only question remaining is the relative disposition of the layers of calcium and carbon atoms with respect to one another.

Now the spheres in alternate layers of the second kind of symmetry considered alone have the relative arrangement of the third kind of symmetry (Fig. 4), and in determining the relative disposition of the calcium and carbon atoms, we may therefore neglect the oxygen atoms, and treat the case as belonging to the third kind of symmetry. The two spiral arrangements in this kind of symmetry, in which the less numerous spheres in the fourth layer are vertically over those in the first (see *ante*), have the necessary symmetry about a single axis, and if the calcium and carbon atoms have one of these arrangements, the requirements of the case are entirely met.

We will now show that the three kinds of atoms can also be arranged symmetrically about a single axis in the first kind of symmetry.

One half the spheres depicting this kind of symmetry will in this case represent the oxygen atoms, and the remaining half the atoms of calcium and carbon (see Fig. 2), and, as previously noticed, the arrangement of either half will be that of the second kind of symmetry. It follows that the question of the relative disposition of the atoms of calcium and carbon is simply the question of the symmetrical arrangement about a single axis of atoms of two kinds present in the proportion 2 : 1 in the second kind of symmetry (Fig. 3). And since the layers of spheres depicting this kind of symmetry have a triangular arrangement (plan *b*), it is evident that this can be accomplished here just as in the former case.

In either of the two arrangements just described we have only to suppose that when the symmetrically placed atoms change volume at the time of crystallisation the dimensions transversely to the axis of symmetry are increased relatively to those in the direction of this axis, and we have an obtuse rhombohedron where formerly we had a cube. And the significant fact that the angle of a rhombohedron of calc-spar diminishes when the crystal is heated supports this theory of its production. Perhaps the arrangement of the atoms according to the first kind of internal symmetry is the more probable of the two, as this would give the cleavage directions coincident with the directions of layers of similar atoms (oxygen).

An important fact supporting our conclusions is that certain definite relations as to their proportions which are found subsisting between the allied forms taken by crystals of the same substance are found inherent in one or other of the five kinds of internal symmetry.

Thus it is well known that if a particular substance is found crystallised in hexagonal pyramids of various kinds—that is, whose sides have various different degrees of inclination to the base—the number of kinds is strictly limited, and they are strictly related to each other. If x be the side of the hexagonal base of the pyramid and y the height for the same substance, while x remains constant, y has not more than fourteen different values, seven related thus: c, $\frac{3}{4}c$, $\frac{1}{2}c$, $\frac{1}{4}c$, $\frac{1}{8}c$, $\frac{1}{12}c$, $\frac{1}{16}c$; and the other seven similarly related thus: d, $\frac{3}{4}d$, $\frac{1}{2}d$, $\frac{1}{4}d$, $\frac{1}{8}d$, $\frac{1}{12}d$, $\frac{1}{16}d$; and c bearing to d the ratio $2 : \sqrt{3}$.

Now, if we turn to the fourth kind of internal symmetry (Fig. 5) to ascertain the possible varieties of inclination of the sides of hexagonal pyramids which can be depicted, we find that the greatest possible height to which we can build a hexagonal pyramid of equal spheres is exactly double the height of a tetrahedron with the same side as the hexagonal base of the pyramid. Thus, if twenty-five spheres form each side of the hexagonal base, giving twenty-four equal distances between the sphere centres in any one side, we find that the highest possible pyramid has forty-nine layers of spheres giving forty-eight equal spaces between consecutive layers.

If we call this height c, it is evident that pyramids corresponding with the first of the above series of actually observed forms will have respectively—

49	layers of balls,	giving	48	spaces between	consecutive	layers.
37	,,	,,	36	,,	,,	
25	,,	,,	24	,,	,,	
13	,,	,,	12	,,	,,	
7	,,	,,	6	,,	,,	
5	,,	,,	4	,,	,,	
4	,,	,,	3	,,	,,	

We find, moreover, that such a series can be readily depicted, and that, upon examination, no additional terms appear admissible.

Again, a further inspection of the stack of spheres shows us that with the same heights—that is, with the respective numbers of layers just enumerated—we may, in place of the base layer which forms a hexagon whose sides have twenty-five spheres each, have a base derived from this in which each of the six spheres at the angles becomes the centre of a side, the outline of the base layer being now a larger hexagon described about the hexagon which bounded the former base layer. The sides of this new base thus bear to the sides of the old the ratio subsisting between the side and the perpendicular of an equilateral triangle, *i.e.* the ratio $2 : \sqrt{3}$. And finally, since the distance between the planes containing the centres in successive layers bears to the distance between centres in the same layer the same ratio which the perpendicular from the angle of a tetrahedron upon its opposite face bears to its edge, that is the ratio $\sqrt{2} : \sqrt{3}$, it follows—

That the two allied series of possible altitudes of hexagonal pyramids thus formed, if we take *the same length of side a* for both, will be—

First Series

$$\frac{2\sqrt{2}}{\sqrt{3}}a;\ \frac{3\sqrt{2}}{2\sqrt{3}}a;\ \frac{\sqrt{2}}{\sqrt{3}}a;\ \frac{\sqrt{2}}{2\sqrt{3}}a;\ \frac{\sqrt{2}}{4\sqrt{3}}a;\ \frac{2\sqrt{}}{6\sqrt{3}}a;\ \frac{\sqrt{2}}{8\sqrt{3}}a.$$

Second Series

$$\sqrt{2}.a;\ \tfrac{3}{4}\sqrt{2}\,a;\ \tfrac{1}{2}\sqrt{2}.a;\ \tfrac{1}{4}\sqrt{2}\,a;\ \tfrac{1}{8}\sqrt{2}\,a;\ \tfrac{1}{12}\sqrt{2}\,a;\ \tfrac{1}{16}\sqrt{2}.a.$$

[1] The very symmetrical form the pentagonal dodecahedron is not in harmony with either of the five kinds of symmetry, nor is it found in crystals.

[2] It has already been remarked that the crystal form of fluor-spar favours the supposition that calcium has half the atomic weight usually attributed to it.

Surely the fact thus established, that each term of a series of relative altitudes of the hexagonal pyramids in which a particular substance crystallises always has to some term of the series thus theoretically derived a particular ratio peculiar to the substance, constrains us to conclude that the above fourteen "root" forms are those to which all crystal forms involving regular six-sided pyramids are referable, and that the actual forms are produced from the "root" forms by difference in the degree of expansion in the direction of the axis of the crystal as compared with other directions at the time of crystallisation.

Other allied forms, as allied octahedra or rhombohedra, can be in the same way connected with some one of the five kinds of internal symmetry.

The peculiarities of *crystal-grouping* displayed in twin crystals can be shown to favour the supposition that we have in crystals symmetrical arrangement rather than symmetrical shape of atoms or small particles. Thus if an octahedron be cut in half by a plane parallel to two opposite faces, and the hexagonal faces of separation, while kept in contact and their centres coincident, are turned one upon the other through 60°, we know that we get a familiar example of a form found in some twin crystals. And a stack can be made of layers of spheres placed triangularly in contact to depict this form as readily as to depict a regular octahedron, the only modification necessary being for the layers above the centre layer to be placed as though turned bodily through 60° from the position necessary to depict an octahedron (compare Figs. 7 and 8). The modification, as we see,

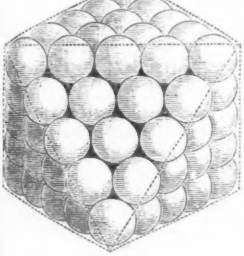

FIG. 7.

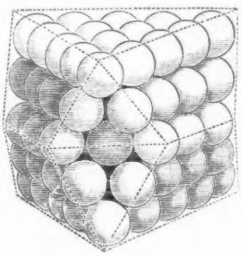

FIG. 8.

involves ***no departure from the condition that each particle is equidistant from the twelve nearest particles.***

Before closing, a few words may be said on the bearing of the conclusions of this paper on *isomorphism* and *dimorphism*.

First, as to isomorphism.

The conclusion that there are but five kinds of internal symmetry possible, three of which indicate a cubic form, evidently accords with the fact that not only the simplest combinations—those in which two kinds of atoms are present in equal proportions—but also many very complicated compounds crystallise in cubes.

Out of the regular system we generally find that for the angles of crystals of different compounds to be the same there must be some resemblance in their atom-composition, and the explanation suggested is that the atoms which are common to two isomorphous compounds, *e.g.* the carbon and oxygen atoms in calc-spar and spathic iron ore, have similar situations in the two different crystals, and that the change of bulk which occurs when crystallisation takes place is due to a change in ***these atoms only***, the atoms not found in both remaining *passive*.

There are, however, some cases which do not at first seem to be met by this view—cases in which the atom composition of isomorphous compounds has only a very partial similarity. Ammonia compounds may be specially mentioned. Thus, ammonic sulphate, $(NH_3)_2H_2SO_4$, is isomorphous with potassic sulphate K_2SO_4.

The following suggestion would seem to enable us to suppose that in this, as in other cases of isomorphism, the phenomenon is referable to the passivity of some of the atoms in the change of bulk which accompanies crystallisation. Let us write ammonic sulphate thus $(NH_3)_2H_2SO_4$, and let us suppose that the symmetrical arrangement is such that the groups, $(NH_3)_2$ just occupy places which might, without altering the symmetry, be filled by additional groups H_2SO_4; that, in other words, the relative position of the groups H_2SO_4 which are present in the symmetrical arrangement is precisely the same as it would be if the entire mass consisted of these groups instead of consisting partly of NH_3 groups. If now, in addition to supposing that in both compounds the active atoms in the process of crystallisation are the sulphur and oxygen atoms, and these only, we suppose that the expansion of some of the atoms of the active kind checks the expansion of others; that only a certain proportion of these atoms expands, we perceive that we may have both the same amount and kind of atom expansion in the two cases, and, as the natural result, isomorphism.

Next, as to dimorphism.

It is evident that a very small change is requisite to convert one kind of internal symmetry into another. Thus we have already had occasion to notice that the only difference in depicting the third and fourth kinds of symmetry is that for the former the centres of the spheres in the first and fourth layers, those in the second and fifth, and so on, range vertically, while for the latter the centres in the first and third, in the second and fourth, and so on, range in this way.

In the case of a dimorphic compound consisting of two kinds of atoms in the proportion of 2 : 1, *e.g.* water, H_2O, we have only to suppose therefore that the same layers of atoms which under one set of conditions produce hexagonal prisms, are by some alteration in conditions arranged in the slightly different way necessary to produce rhombohedral forms. Other cases of dimorphism are probably to be accounted for much in the same way.

Thus the following interpretation of the fact that calcic carbonate, which we have seen crystallises in obtuse rhombohedra as calc-spar, sometimes crystallises in six-sided trimetric prisms as aragonite may be offered.

We have already endeavoured to show that the first or second kind of internal symmetry is that proper to calc-spar. We will now endeavour to show that the fifth kind of internal symmetry (Fig. 6) is proper to aragonite.

Alternate layers of spheres (plan *b*) will represent the oxygen atoms, and the other alternate layers the calcium and carbon atoms; the central layers of the triplets above alluded to, viz. the second, the fourth, the sixth, &c., being the oxygen layers; the calcium and carbon atoms in the remaining layers will be symmetrically arranged (plan *f*). From the fact of the crystals being trimetric, the layers containing the last-named atoms, which, considered apart from the oxygen layers, are in the fourth kind of symmetry, probably have the arrangement above described, in which the less numerous spheres form zigzags, the stack in this case having a different symmetry about three axes at right angles to each other (Fig. 6).

The fact that the dimorphic varieties of the same substance have different densities is in harmony with the supposition that different sets of the atoms are concerned in the different cases; that the active atoms which produce one form are not those, or those only, which produce the other.

It is not always necessary to refer two incompatible crystal forms of the same substance to two different kinds of internal symmetry: for example, from the third kind of internal symmetry we can produce square-based octahedra, and we can also produce right-rhombic prisms, and in accord with this we have the well-known fact that right-rhombic prisms of sulphate of nickel, $N_2SO_47H_2O$, when exposed to sunlight are molecularly transformed, and, though they neither liquefy nor lose their form, when they are broken are found to be made up of square-based octahedra several lines in length.

WILLIAM BARLOW

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following awards (among others) have been made at St. John's College on the results of the examination for candidates who have not yet commenced residence:—

For Mathematics: H. F. Baker (Perse Grammar School, Cambridge), Foundation Scholarship, raised for two years to 75*l*. a year; A. W. Flux (Portsmouth Grammar School), Minor Scholarship of 75*l*. a year; P. T. Fagan (Highwood School, Weston), Exhibition of 50*l*. a year; H. R. Norris (University College School), Exhibition of 30*l*. a year.

For Natural Science: G. S. Turpin (Nottingham High School and Owens College, Manchester), Foundation Scholarship raised for two years to 75*l*. a year; P. Lake (Newcastle College of Science), Minor Scholarship of 75*l*. a year; W. Harris (Bradford Grammar School), Exhibition of 50*l*. a year; W. M. Mee

(Trinity College, Dublin), Exhibition of 32*l*. a year [Mathematics and Physics].

For Hebrew: G. C. Ewing (Merchant Taylors' School, London), Exhibition of 33*l*. 6*s*. 8*d*. a year.

SOCIETIES AND ACADEMIES

London

Royal Meteorological Society, December 19.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—The following were elected Fellows:—R. Bentley, W. Bonallo, Miss E. Brooke, Rev. A. Conder, T. H. Cowl, J. A. W. Oliver, C. M. Powell, W. B. Tripp, and Fung Yee. The papers read were:—On the explanation of certain weather prognostics, by the Hon. Ralph Abercromby. The author explains about forty-four well-known prognostics belonging to the following groups —(1) diurnal; (2) sun, moon, and stars; (3) sky; (4) rain, snow, and hail; and (5) wells, springs, and coal-mines—by referring them to the isobaric conditions in which they are observed. By this means he is able to indicate the circumstances under which any prognostic fails, as well as those under which it succeeds.—Preliminary inquiry into the causes of the variations in the reading of black-bulb thermometers *in vacuo*, by G. M. Whipple, B.Sc. It has long been known that there is a want of accordance between the different instruments used for measuring the intensity of radiation, and with a view of ascertaining the cause of the variations in the readings of the black-bulb thermometers *in vacuo*, the author has made a comparison with a number of these thermometers, the results of which are given in the paper. It is shown distinctly that the effect of an increased coating of lamp-black on the bulb is to raise the temperature, and also that the size of the thermometer-bulb is a most important factor in the case of this instrument.—Report on the phenological observations for 1883, by the Rev. T. A. Preston, M.A.—Mr. J. S. Dyason exhibited a series of coloured sketches illustrating the recent atmospheric phenomena during November and December.

Geological Society, December 5.—J. W. Hulke, F.R.S., president, in the chair.—George Jonathan Binns, Horace T. Brown, James Dairon, Rodolph De Salis, Hugh Exton, John Forrest, Prof. Bernard J. Harrington, James Patrick Howley, John Sylvester Hughes, Prof. George T. Kennedy, Rev. Arthur Noel Malan, Robert Sydney Milles, Edwin Radford, Edward Pierson Ramsay, William Henry Rands, Thomas Roberts, Joseph Ridgway, and Harry Page Woodward were elected Fellows of the Society.—On the Cambrian conglomerates resting upon and in the vicinity of some pre-Cambrian Rocks (the so-called intrusive masses) in Anglesey and Carnarvonshire, by Henry Hicks, M.D., F.G.S. In a former paper the author had maintained that there was no evidence to show that the so-called intrusive granite in Anglesey had altered the Cambrian and Silurian rocks in its immediate vicinity, or that they had been entangled in it as described, but that it seemed to be a rock of metamorphic origin, varying much in its general appearance at different points. He contended that, instead of being an intrusive granite, as supposed by the officers of the Survey, it was in all probability the oldest rock in Anglesey. The basal Cambrian conglomerate in contact with it is in an unaltered condition, and at Llanfaelog contains an extraordinary proportion of well-rolled pebbles, identical in mineral composition with the so-called granite immediately below. Fragments of all the varieties of rock found in the granitoid axis are recognisable in the conglomerate, and in precisely the same condition as in the parent rock. Fragments of the various schists of the area were also present; so that he thought there cannot be the shadow of a doubt that the so-called granite and the metamorphic schists are older than the conglomerate, and therefore pre-Cambrian. The view maintained by the Survey that the schists are altered Cambrian and Silurian strata, and the granitoid rock an intrusive granite of Lower Silurian age, is consequently quite untenable. In Carnarvonshire equally conclusive evidence was obtained from many areas. Fragments of the Dimetian (Twt Hill type) occurred abundantly in the basal Cambrian conglomerates at Dinas Dinorwig, Pont Rothel, Moel Tryfane, and Glyn Llifon. Quartz-felsite pebbles in every respect identical with the varieties found in the so-called intrusive ridges between Bangor and Carnarvon, and to the north and south of Llyn Padarn, were found on the shores of the Menai Straits, in the railway-cutting at Bangor, at Liandeiniolen, Dinas Dinorwig, Llyn Padarn, and elsewhere. This evidence, supplementary to that previously furnished by Prof. Hughes, Prof. Bonney, and the author, is conclusive as to these areas, since the basal Cambrian conglomerates, which are in contact with these supposed intrusive masses, are composed almost entirely of rocks identical with the latter; and this could not possibly be the case if the granitoid masses had been intruded among the conglomerates after their deposition.—On some rock-specimens collected by Dr. Hicks in Anglesey and North-West Carnarvonshire, by Prof. T. G. Bonney, F.R.S., Sec.G.S. The author stated that pebbles in the blocks of conglomerate collected by Dr. Hicks to the north of Llanfaelog were practically undistinguishable macroscopically and microscopically from the granitoid and gneissic rocks which occur *in situ* between that place and Ty Croes, and that the matrix contained smaller fragments, probably from the same rock, with schist bearing a general resemblance to members of the group of schists so largely developed in Anglesey, and with grits, argillites, &c. Pebbles of granitoid aspect in the Cambrian conglomerate near Dinas Dinorwig, &c., bear a very close resemblance to the Twt Hill rock, and are associated with abundant rolled fragments of rhyolite resembling those already described from the Cambrian conglomerate and the underlying conglomeratic beds and rhyolites. Two pebbles of rather granitoid aspect in the Cambrian conglomerate by the shore of the Menai Straits, near Garth, prove to be spherulitic felsite, somewhat resembling that already described by the author from Tan-y-maes. He pointed out that the evidence of these specimens collected by Dr. Hicks, added to that already obtained, led irresistibly to one of two conclusions—either that, when the Cambrian was formed, an area of very ancient metamorphic rock was exposed near Ty Croes and in the Carnarvonshire district, or that the rhyolitic volcanoes were so much older than the Cambrian time that their granitic cores were already laid bare by denudation. Hence, in either case, the existence of Archæan rock in North Wales was proved. To one or other of these conclusions he could see no possible alternative, and he considered the former to be (even if some of the granitoid rock were granite) far the most probable.—On some post-Glacial ravines in the Chalk Wolds of Lincolnshire, by A. J. Jukes-Browne, F.G.S.

Edinburgh

Mathematical Society, December 14.—Mr. Thomas Muir, president, in the chair.—Mr. J. S. Mackay read a paper on the medioscribed circle of a triangle with its analogous and associated circles viewed from their centres of similitude.—Prof. Chrystal stated some propositions in geometry for which he wished proofs.—Mr. Muir made a communication on determinants with *p*-termed elements.—The Secretary gave a new construction by the Rev. G. McArthur for Euclid ii. 9, 10; and Mr. James Taylor Dollar proposed for solution a theorem in elementary geometry.

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